

39843/p To the Cholera Committee of the  
College of Physicians with  
the Author's respects.

AN INQUIRY INTO THE BEARING OF THE  
EARLIEST CASES OF CHOLERA, WHICH  
OCCURRED IN LONDON DURING  
THE PRESENT EPIDEMIC,

ON THE STRICT THEORY OF CONTAGION.

FROM NO. VII OF THE  
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BY

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## AN INQUIRY,

&amp;c.

[THE following Inquiry originated in a request made to Dr. Parkes, by the General Board of Health, that he should examine into the evidence which might be derived for or against the doctrine of Contagion, by an analysis of the early cases of cholera in London. It is here published, with the cases abridged, in the belief that the evidence brought forward in it will be acceptable to the profession, and will be found to have an important bearing on the great question at issue.]

I propose to give in this Report the chief facts which I have been able to collect, regarding the early cases of cholera in London; in order that some decision may be formed as to the mode in which these cases originated, whether from a poison emanating from the bodies of other persons labouring under the same affection, from a poison introduced in any other method, or from a poison actually generated in London itself.\*

It is universally and truly considered, that the inquiry into the origin of the first cases of an epidemic disease, in any locality, is a necessary preliminary to all other inquiries respecting the origin of future cases. At that period of the epidemic, the question is reduced into as simple elements as we can ever hope to find it in; and the influence of essential antecedents is less obscured than at a later date, by the presence of accidental and unnecessary circumstances.

In order that the terms which I am about to use may be correctly defined, and that it may be clearly understood in what method I am about to investigate this subject, and in what light I regard the general aspect of the great question of the nature of contagious and epidemic diseases, I shall commence with a condensed statement of what I consider to be the most prominent and correct opinions at present entertained by medical men, respecting the diffusion and mode of propagation of those diseases, which are generally allowed to arise from specific and uninterchangeable poisons, and which are capable, under certain conditions, of becoming so prevalent as to be entitled to the appellation of *epidemic*. This statement will be, in fact, a general definition; and when completed, the bearing of the subsequent argument will be at once apparent.

All the opinions of the day, however widely different they appear, may, I believe, be comprised under two separate creeds: that of the strict, and that of the modified contagious theory.

\* I assume that cholera, like other epidemic and contagious diseases, must result from the action of a specific agent, rather than from any temporary combination of atmospheric influences. I believe that satisfactory reasons may be given in support of this assumption. Into these I do not wish now to enter; but as far as cholera is concerned, I may refer to the second Volume of the 'British and Foreign Medico-Chirurgical Review' (p. 93), in which this question is shortly discussed.

The *strict contagious theory* I take to be that which refers epidemic diseases to the action of specific poisons, which (it alleges) multiply themselves only during their passage through the animal body. All other reputed modes of increase this doctrine considers to be doubtful or untrue; and it looks upon the external circumstances which surround the animal frame as influencing the efficient cause or poison of the epidemic, only so far as they render the body a more or less fit recipient for its action. As it concludes that the body is the only source from which a fresh supply of the specific agent can be evolved, it deems it necessary that the person, to be infected, should come within the influence of the particles of poison (imparted by contact, diffused in the air, or adhering to clothes) which have been emitted from the breath, surface of body, or excretions of an individual already suffering from the disease, or from the corpse of one who has already died of it. Nor can it be said that this view is otherwise than philosophical; that is to say, it is rested on a foundation of undeniable truth, and its inferences are not obviously inconsistent with the premises it lays down. It sprang naturally, indeed, from a recognition of the great truths, that each epidemic disease originates from a cause which is peculiar to itself, and which is not interchangeable with the cause of another epidemic disease; that, however two or more such diseases may be temporarily combined, they are yet fundamentally distinct; that they are governed by separate laws, and display attributes which manifestly prove their non-identity. And it so happened that when this opinion first took solid root in medical literature, the epidemic diseases which were chiefly witnessed in France, England, and even in Italy, did really spring from poisons whose most potent, and apparently whose only source, was in the very bodies, whether of men or of animals, which were suffering from their effects. The poison grew at the expense of that it tainted. At least, this was eminently the case with smallpox, and with measles and scarlet fever, which were sometimes distinguished, sometimes confounded together. It was also generally presumed to be the case with the Levant plague, which then had received very little study, except in the countries in which it was comparatively an infrequent and transient visitor.

And as in some of these cases the poison was actually tangible, could be procured in substance (though not in a state of absolute isolation), could be carried on the point of a lancet, and transferred from one body to another, it was not really a greater assumption than was warranted by the facts then known, to infer that the more volatile or intangible poisons, which were not inoculable, and the argument for whose existence was indeed founded only on analogical reasoning, did in reality multiply only in the same manner as did those poisons whose existence was demonstrable otherwise than by their effects on the human system alone.

During the last sixty years, however, the study of several diseases imperfectly known to the older physicians has added so many new facts to our knowledge of the several specific epidemic diseases, that the strict contagious theory has been insensibly undergoing alteration, until in the present day it bids fair to become merged in a higher generalization.

The extension of commerce and the military occupation of colonies have made us better acquainted with the several forms of fever, which in the West Indies, and on certain parts of the African coast, have an endemic and a local origin, but which also possess the power, under certain circumstances, of increasing themselves in the human body. The birthplaces of the Oriental plague, Egypt and Syria, have been traversed by the military and civil surgeons of France and England; and the progress of science and of national intercourse has enabled us to note more fully the returns, and to interpret more correctly the attendant phenomena, of that catarrhal fever, which, under the name of influenza, la grippe, &c., so frequently pursues its evanescent course over the greater portion of the world. The more accurate investigation of the present day has, particularly in the last twenty years, opened up fresh points of view, under which the several

forms of fever prevalent in this country might be studied. While the different affections which were formerly described (under the name of continued fever) rather as varieties of a single entity, than as separate and uninterchangeable diseases, are now considered with great probability to be the products, not of a single poison, but of two or more, the influence of these several poisons has been studied, not simply as by the older physicians during their action on isolated patients, but with reference to classes of individuals. Thus it has been discovered that the circumstances under which these classes are placed, exercise an astonishing influence in aiding or counteracting the entrance of the specific agent into the body, and in favouring or nullifying the vigour of the action it there exerts.

The tendency indeed of all these observations has been to show—1st, that the effect of the human body, as a single and uniform element in producing changes in the morbid poisons, and in thereby assisting their diffusion, is by no means so great in these instances as it is in the cases of smallpox and scarlatina; 2d, that it is most probable that these poisons are in the first instance derived from sources foreign and extrinsic to the human frame, and are subsequently more or less frequently propagated and multiplied by means which are also foreign and extrinsic to it; and 3d, that the degree in which they multiply in the human body, and are in this way propagated, varies in the case of each particular poison, and according also to its alliance with other poisons, or to the state of the system through which it passes.

The evidence on which this opinion is founded cannot, of course, be here discussed; but it may be stated that all the later researches on plague, yellow fever, and in a less degree on the fevers of this country, have tended to strengthen this position, both in respect to the amount and to the precision of argument by which it is supported.

To assimilate these observations, and to accommodate their dogma to the new truths which were inconsistent with the rigor of its previous enunciation, the advocates of the strict contagious theory have introduced various changes, and have modified their modes of expression according to the genius and temperament of each individual advocate.

Such advocates advance the following considerations in reply to the statement above given:

In the case of tropical fevers, they seek to break the link which has been made between marsh and contagious yellow fevers, by considering the latter a peculiar disease, and by placing the specific poison which springs from marshes in a distinct category of morbid agents. They deny the validity of the evidence by which the contagious yellow fever (for such undoubtedly exists) is supposed to be traced up from fevers arising from endemic sources, and which evidence, if true, would necessarily place the poisons of all these affections in the same class.

While the influence of epidemic constitutions of the atmosphere, and of external local circumstances, over the fevers of this country, as over the Oriental plague, is admitted, these conditions are presumed simply to augment the susceptibility of the subject, but to have no direct influence on the virus itself. They act on the soil which is to receive the plant, but they do not heighten the inherent force which is normal to the seed. To explain certain special cases, which seem strongly opposed to such a view, and which point to the development *de novo* of the specific virus, the advocates of the strict contagious theory have attributed an extraordinary durability to the poisons of fever and of plague, and a power of lurking within the frame unaltered until the accessory causes have prostrated the force which at first successfully resisted them.

Influenza, again, which is a disease manifestly arising from a special virus, and which attacks so many individuals at once, and is propagated with such rapidity that we can hardly suppose this to occur through the influence of the human body, has not yet been fairly considered by the strict contagious theory. If it

is not allowed to modify this theory, it must be shut out altogether from this class of morbid poisons, although in many respects it is an apt representative of the class.\*

The strict contagious theory was considered to derive its strongest proofs from the fact, that those persons only suffered who came in contact or were in proximity with the sick. Lately, however, a section of this party has questioned the possibility of setting bounds to the diffusion-distance which a poison can traverse before its powers are destroyed. Whilst the older contagionists never assigned a greater distance than a few yards, this new opinion (which still looks on the human body as the *fons et origo mali*) doubts whether the poison may not float for an indefinite distance through the atmosphere, the condition of which may favour or impede its volatility and rapidity of diffusion. Consequently as regards this party, the evidence which is considered decisive against contagion is not decisive against them, or at least they do not consider it so; although it may be demanded how (independent of inoculation) they could prove that a single morbid poison was ever reproduced in its passage through the body, unless by the evidence derived from the fact that those about the sick are more liable to the disease than those who are not?

Thus, at the present time, two great opinions seem to divide the medical world, and to be contending for the mastery. Each opinion has the same foundation—each looks on epidemic diseases as produced by specific and uninterchangeable agents—each admits that some of these agents evidently propagate themselves chiefly, or possibly solely, by means of their action on the body. After this, their paths divide. What they both assume to be true of certain specific agents, the strict contagionist affirms to be true of all of them; the modified contagionist affirms it to be true of certain of them, only in a limited and conditional sense.

The latter party, in seeking to determine the mode of non-contagious propagation, considers with the greatest attention the media external to the human body, which surround the particles of any special poison. It regards the humidity and temperature of the air, its purity, its electrical condition, the weight of its column, and the movement of its masses. It questions what may be the exhalations from the soil, from decaying substances, from all the various natural or artificial peculiarities which vary the surface of the ground. In all these circumstances it sees a twofold action—an action upon a virus introduced among them—an action upon a human frame submitted to them. It seeks to determine what relative assemblage of these conditions is most favorable to the spread of a poison; it believes that, under its favouring conditions, some poisons introduced *ab externo* may augment by reproducing themselves. It goes even farther than this; in certain cases, it sees, in an intense concentration of these several circumstances, a development *de novo* of that specific poison, which is proper to that particular assemblage of conditions. On the banks of the Ganges it witnesses the cholera poison spring into existence; in the swamps of Batavia, the malignant intermittent passing into remittent; on the river marshes of Western Africa, the deadly remittent, from which springs the epidemic yellow fever; among the effluvia emitted from our great cities, or among the miserable hovels of a starving nation, the fever-poisons of this country; among the Fellahs of Egypt, with their peculiar rites of sepulture, and with their pernicious customs of social life, the poison of bubo-plague. It attaches, also, much importance to the consideration of the occasional, and even periodical, augmentations in the

\* If influenza arises from any peculiar allotropic condition of the oxygen of the atmosphere produced by electrical vicissitudes, and does not originate from any agent allied to the ordinary contagious agents, it may be dismissed from consideration; but, at present, the production of ozone during influenza epidemics, the capability of ozone to produce the peculiar symptoms, and the other questions which would prove a connexion between the two, are all matters of the merest speculation. On the other hand, the general laws of influenza, the peculiarities of its course, and even its occasional irregularities, render its separation from the poisons of cholera, of some forms of marsh fever, and perhaps of pertussis, almost impossible.

vigour of the specific poisons, believing that these are partly dependent on influences exerted on these poisons by "epidemic constitutions" of the atmosphere.

While the followers of this creed admit that some or all of these specific poisons may be truly contagious—that is, may be reproduced by the human system—they are disposed to limit this mode of propagation, and to admit it no farther than it can be proved by actual evidence. They refuse the argument from analogy, that because smallpox is manifestly augmented by the body, any other disease is necessarily subjected to the same influence. In the case of several poisons, they advance evidence to prove that this mode of increase is exceptional, and comparatively unimportant.

Finally, in conformity with their creed, the supporters of this opinion seek to arrest the progress of epidemic diseases by destroying the external local conditions, which, with perhaps some general atmospheric conditions, are assumed to be the necessary pabula for the poison, and by removing the internal states of the system which predispose to the reception of the cause.

The strict contagious theory, on the other hand, considers these opinions as overstrained; it looks primarily to the human body as the focus for the poison, and to the external conditions as simply predisposing and accessory causes. It questions the doctrine of origin *de novo*, and contenting itself with the fact that the poisons exist, doubts whether it is possible to trace them to their origins. It avoids, indeed, this question as beyond its reach. It separates such poisons as have a determined local origin from the true contagions, and denies that they can ever become epidemic. It places its great reliance, not on sanitary preventive measures, though for the reasons above given it does not deny the wisdom of these, but on isolation and quarantine. By preventing proximity and exposure, it deems that the poison must become extinct, if it meets with no animal frame in the structure of which it may increase. Consequently, to push this view to its legitimate conclusion, a perfect system of quarantines would secure to a nation complete immunity from all epidemic diseases, except those which were previously located within it. To be perfectly consistent with itself, this theory must restrict the action of the so-called "epidemic constitution" to a simple effect on the recipient of the poison.

It will be seen at once that these two opinions are in one sense opposed to each other, in another are even compatible. The former includes the latter, or might do so; for it admits the human body as a possible multiplying source, in the case of all the poisons. It only demands that the proportion of cases which increase in this way, shall be proved from evidence similar to that which proves the production in cases from other sources. And according to the peculiar mode of regarding evidence in each individual mind, the supporters of this opinion are divided into various subsections, who all more or less incline, but none of whom actually reach (except perhaps as regards the case of influenza), the extreme of non-contagion. If in some cases (as in influenza) the evidence of the increase of a poison in the body is so defective as to cause doubts whether it ever occurs, still it is admitted that such negative evidence is always to be received with the reservation, that peculiar conditions may yet remain to be discovered or noted, which may really have the effect of temporarily increasing the influence of this particular source of the poison.—The strict contagious theory, on the other hand, is not capable of this expansion and amalgamation. It repels, and indeed necessarily and completely excludes, everything which is not comprised within its definition, or is not capable of being added to that definition without impairing its fundamental tenet. It cannot, in fact, abandon its doctrine that the body is the only multiplying agent of each poison, without entirely surrendering the character which stamps its individuality. Immediately that a partisan of this opinion allows that the impure and fetid atmosphere which hovers over a fever-locality, can generate a single particle of fever-poison, or can cause a particle thrown off from the body to generate another like itself,

he has undermined his theory ; and the question between the relative propagating powers of the body and the surrounding media, becomes merely a question of degree.

Such are, I believe, when stated with the utmost brevity and simplicity, and divested of all qualification not absolutely essential, the two great opinions of the day respecting the causes of epidemic diseases. There can be no doubt that in a scientific and a commercial point of view, the debate is one of great importance, and it seems probable that it is now gradually approaching a final solution.

There is one disease to which I have avoided making any but the faintest allusion, but the investigation of which has contributed essentially to the reception of the modified contagionist view. Asiatic Cholera belongs manifestly to the most strongly marked order of epidemic diseases. Its symptoms are remarkably precise and recognisable, and their course and mode of evolution are determinate ; its poison propagates itself at intervals over countries which it afterwards abandons for a series of years. There is, therefore, in these countries nothing of the endemic or the local about it.\* It sweeps, like an atmospheric wave, over vast tracts of the earth, or possibly itself brings or induces the epidemic constitution which permits its poison to be continually generated. There is little doubt but that it nosologically belongs to the same order of diseases as smallpox or typhus—viz. that it is produced by a definite epidemic poison entering the person *ab externo*.

As, therefore, it is a disease evidently not indigenous to this country, as it comes to us from without, and merely borrows force from the elements of increase it finds here, whatever these may be, it affords a most convenient opportunity for applying the two theories above described, and for observing which best interprets its phenomena. And as every point which bears on Asiatic Cholera bears also on the general subject of contagion, it is of the greatest importance to render all evidence as correct as it is possible to make it.

In the following Inquiry, I do not intend to enter on the general subject. I am simply desirous of placing on record the evidence respecting the first cases of cholera in London, and to inquire particularly into the possibility of these cases having arisen from contact or proximity with persons already diseased.

The strict contagionist theory has always considered that strong evidence in its favour was to be obtained from a study of the early cases. It claims to be always able to point out the channel of introduction, and to trace the first steps of the malady ; afterwards, when the sick have become numerous, it considers that persons may be exposed to emanations, and may sicken, without being aware that they have been so exposed. At this late period, therefore, the proofs of contact or proximity cannot be obtained ; and negative evidence loses its value.

The first twenty-five or thirty cases are then, in this point of view, most important. Did they arise together, or near each other ? Were they exposed to sources of contagion, from which the other inhabitants of the district were exempt ? Can each successive case be traced to a prior case, until the patients are too numerous to be followed up ?

If these questions can be answered in the affirmative, it must be conceded that the strict contagionists have carried their point ; if they are not so answered, then the observer has to seek for the cause of the early cases in other directions.

It is necessary in examining evidence on this point to adopt two precautions.

1. Every reputed case of the disease must be known.

\* I assume here, that common English cholera is an entirely different disease from Asiatic cholera. This point is not fully ascertained ; but at present the weight of the argument is against the identity. But, even if we admitted that every year sporadic cases of true cholera occurred in London, still the bearing of the argument in the text would be unaffected. The epidemic has certainly on two occasions travelled regularly to us, and has not arisen into activity among us independently of an external and peculiar influence.

2. Every reputed case must be inquired into, and its exact nature determined; the loose accounts of by-standers and non-professional persons not being received as credible evidence.

With regard to the first of these precautions, there is little doubt that the cases I am about to detail comprise all, or nearly all, the first cases in London. Almost all the early cases in London were reported to the General Board of Health, and were immediately investigated by means of their agents; and if any early case was not so reported, and proved fatal (which in the case of cholera it would probably do), it would be found in the Registrar-General's returns.

As to the second precaution, I have to state that the General Board, as already said, having, by means of competent agents, inquired into all the cases, no loose hearsay evidence will have even to be discussed. I have myself carefully examined the records of all these cases; in many instances I saw the patients themselves; and any case which seems doubtful will be left out of the argument, unless its admission would seriously modify a conclusion come to in its absence, in which case I shall discuss the point both with and without it.

It has been stated in some of the medical journals, that several cases, very much resembling Asiatic cholera, occurred in London during the summer and early autumn months of 1848. The only decided case of this kind which I can find, however, is one reported by Mr. Haden, in the 'Medical Gazette' for October 13, 1848, to which journal I must refer for its details.

**CASE I.** This case, at the time of its occurrence (July), was considered by Mr. Haden to bear so strong a resemblance to Asiatic cholera, that he felt inclined to believe it to be an advanced case of the approaching epidemic of that disease, and not to be simply a case of the common English or bilious cholera. If it was, however, a severe case of common sporadic cholera, it becomes a question by what symptoms it can be distinguished from the epidemic disease. The assemblage and concatenation of symptoms were altogether similar to those of Indian cholera. The evacuations were like congee water, and without bile; there was the usual failure in the circulation, with its accompanying symptoms; there were the copious sweats and the suppression of urine; there were the usual periods of the disease, vomiting and purging, soon attended by cramps, a subsequent superadded cold stage, and a period of reaction. It is clear that had this case occurred at a time when epidemic cholera was prevalent in the neighbourhood, it would unhesitatingly have been considered a marked example of the disease.

As, however, I am anxious to avoid all possible fallacies, I shall put aside this case, or rather shall not use it in the present inquiry. Whatever its nature, it cannot be attributed to any poison brought near the patient by an infected person. There had been no case of cholera in the neighbourhood, and no one subsequently suffered.

**CASE II.** The second case was reported to the Board of Health by Mr. Howell, of Wandsworth. The patient, a man aged 41, a coalwhipper by trade, living at Spencer's court, Waterside, Wandsworth, was attacked at 11 p.m., July 31, with violent purging and vomiting, and severe cramps; these symptoms were speedily succeeded by blueness, lividity, and coldness of the surface and tongue. He remained in the cold stage for forty-eight hours, and then passed through slight consecutive fever.

Although the symptoms above given are well marked, and are detailed by a practitioner, who it is believed was well acquainted with the symptoms in the epidemic of 1832, still, as cases similar to this one are said by some practitioners to be of annual occurrence in London, and in other parts of the country, it will be advisable to omit this also from the present inquiry. It may be mentioned, however, that in this very locality a considerable number of cases of undoubted Asiatic cholera occurred during the last month of 1848, and the first month of the present year.

**CASE III.** The third reputed case in London, with which I am acquainted,

occurred in the practice of Mr. Fairbrother, of Southwark, on the 16th of September. A man, named John Dean, aged 53, resident at No. 5, Lion street, New Kent road, died after a few hours' illness. An inquest was held, as there was a suspicion of poisoning; and Mr. Fairbrother gave it as his opinion that the patient died from Asiatic cholera.

Although I could not undertake to say positively what this case may have been, and although Mr. Fairbrother, who saw the case, is more likely to be correct than any one who had not that advantage, it does appear to me that some doubts may be entertained whether it was really Asiatic cholera. Without, however, deciding on this point, more particularly as I am not aware whether a post-mortem examination was made, it will be advisable not to use this case. It was not supposed to be traceable in any way to contagion, and therefore its omission is no prejudice to the contagionist argument, but rather the reverse.

CASE IV. The fourth case of cholera which occurred in London, was the following.

The Elbe steamer left Hamburg on the 22d of September, and arrived in the river on the 25th. A seaman, named John Harnold, left the vessel, and went to live at No. 8, New lane, Gainsford street, Horsleydown. On the 28th of September he was seized with symptoms of cholera, and died in a few hours. It is stated in a letter to the General Board of Health, from Mr. Russell, who attended the patient, that all the characteristic symptoms of Asiatic cholera were present. Mr. Bowie, who inquired on behalf of the Board into the particulars of the case, corroborated this statement. This may then be considered as an undoubted case of cholera.

It becomes a question of some interest to determine whether this man became infected at Hamburg, where cholera was raging at the date of the sailing of the vessel, or in Horsleydown itself. Some support is given to the first opinion by the fact that another probable case of cholera had occurred on board the steamer at sea.

Mr. Bowie's Report gives the following particulars of this case:

"On the voyage from Hamburg to London, and about forty miles from Lowestoft, the second engineer, who had been long in ill health, died of an attack very much resembling cholera. Immediately after death, the body was placed in a box which was closely nailed down. All the clothes and bedding were thrown into the sea. When the vessel arrived at Gravesend, she was detained about six hours, during which time the quarantine doctor came on board, and granted permission for the body to be conveyed to London to be delivered to the friends, which was done at St. Katharine's Docks by the first engineer."

It may be questioned, however, whether the statement of the captain, "that the disease very much resembled cholera," is of much value, particularly when it is remembered that this man had been for some time in bad health. But if this were a case of cholera, it would be important, as proving satisfactorily that the crew of the Elbe had been exposed at Hamburg to the influence of the choleraic poison. The *certainty* thus afforded us that John Harnold was exposed in that locality to the action of the cause, would necessarily increase the probability that he was there infected. It may be said also that the time between the departure from Hamburg, and the occurrence of the disease in London (six days), was within the limits of the incubative period; though certainly it is rather a long time.

On the other hand, there is little doubt, from the facts to be presently adduced, that Harnold, when he entered London, entered a place where cholera was commencing to prevail; as cases, contemporaneous or nearly so with his own, occurred in two or three distant parts of the metropolis. And as it seems to be a peculiarity of this singular disease, that journeys from infected places increase the liability of the system to suffer from its cause, it may be doubted whether it is not as likely to be the fact, that this man became infected during the three days that he was on shore, before the disease appeared.

As far as the argument is concerned, however, it is of no importance which view is the most correct. If the disease was imported thus from Hamburg, it did not spread in Horsleydown. Two days subsequently, indeed, Mr. Russell was sent for to a patient in the same house, who fancied he had cholera; but on examining into particulars, it turned out that the individual in question had been greatly alarmed at the death of the seaman, and was suffering more from the effects of fear than anything else. He was quite well in a few hours. No other person was taken ill in the house or immediate neighbourhood; although if the second case had not been inquired into, a vague story of communicated disease might have arisen in the neighbourhood.

CASE V. Within 24 or 36 hours after the death of John Harnold, an undoubted case of cholera occurred in another part of London, in Lambeth, which is distant about two miles from Horsleydown. The patient was a man named John Murphy, aged 22, resident at No. 26, Lower Fore street, South Lambeth. He was a labourer in the adjoining gas-works, but out of employ, and badly fed. On the 30th of September he had been with a cousin to Kensal Green, on the Harrow road. He appeared in the morning to be perfectly well; but was first seriously attacked at 11½ p.m., and died at 8½ on following morning (Oct. 1).

The correctness of the account given to me by the friends of the patient, when, by order of the Board, I inquired into this case, was corroborated by Mr. Thomson, of Lambeth, who attended the patient, and who at once considered the case to be one of Asiatic cholera. After death this man became very stiff. When I inspected the house five days after his death, he was still unburied. I removed the lid of the coffin, and found that, though decomposition was far advanced, and the face was swollen and black, the cadaveric rigidity was yet well marked in the extremities. That this was a genuine case of Asiatic cholera, I have no hesitation in affirming.

At the same time that this case occurred in Lambeth, the first of a series of cases appeared in a small court in Chelsea, situated on the opposite side of the river from Lambeth, and still farther removed from Horsleydown. In the course of a few days, six cases had occurred in this narrow court, which is situated close to the river, and is one of the most wretched and dirty localities in Chelsea. These cases were attended by Mr. Keen, of Chelsea, and were also visited by numerous medical men, who were all satisfied that they were undoubted examples of Asiatic cholera.

CASE VI. Richard Cook, aged 10. This case, the first of the series just referred to, was certainly very mild in character; yet the symptoms, and the occurrence of better-marked cases immediately afterwards in the same locality, seem sufficient to establish the fact of its being one of cholera.

CASE VII. The next case occurred in a locality widely removed from any of those formerly described, viz. in a court leading out of Fleet street. The subject of it was Jane Langham, aged 27, who resided at No. 3, Harp court, Fleet street. She was visited by Mr. Digby, of Fleet street; who, when first summoned to the patient, considered her to be in the collapsed stage of Asiatic cholera; and his diagnosis was fully borne out by the general symptoms.

CASE VIII. The next occurred in a part of the town several miles removed from any of the former localities.

Owen Jones, æt. 60, a convict on board the Hulk *Justitia*, lying off Woolwich, was admitted into the Convict Hospital-ship *Unité*, also lying off Woolwich, at 7 p.m., October 2d. It was ascertained that he had eaten his dinner as usual, and that soon afterwards he appeared chilly and indisposed. At tea-time he had spasms in the arms, and his shipmates becoming alarmed at this, and at the cold listless state into which he seemed to be falling, sent him to the hospital. When admitted he was evidently in the collapsed stage of cholera, and died after seven hours.

I was informed by Mr. Dabbs, R.N., on duty on board the *Unité*, that this

case was undoubtedly one of cholera. It was apparently a case of the most malignant type, in which fibrine is poured into the intestinal canal with very little of the water and soluble salts of the blood. The diagnosis of the case was also confirmed by the speedy occurrence of other cases of cholera, presenting symptoms very similar.

**CASE IX.** The next case occurred in a house three doors removed from the one in which Case V had occurred.—James George, aged 40, residing at No. 29, Lower Fore street, Lambeth; a butcher's carter.

On Wednesday, October 4th, he got up at his usual time, viz. 4 a.m.; and immediately complained of pain in the bowels; he passed suddenly a very loose and offensive evacuation. Before 5 o'clock he had two or three more evacuations, but certainly not more than this; he did not vomit or complain of noise in the ears, giddiness, or faintness. His wife was so accustomed to see him affected with bowel complaint, that she paid little attention to his symptoms, and, after taking a dose of brandy and rhubarb, he went to his work in the city. On his way to Newgate Market, he was attacked in St. Paul's Churchyard with violent vomiting and purging; the latter symptom continued until his admission into St. Bartholomew's Hospital at half-past 8 a.m., where he died at 9 p.m., having been ill seventeen hours from the period of the first stool.

The physicians at St. Bartholomew's were unanimous in considering this an undoubted case of Asiatic cholera, and reported it as such through the treasurer of the hospital to the Secretary of State.

It should be mentioned that this man was not acquainted with Murphy (Case V), nor with any one in the house (No. 26) in which Murphy lived. If the two men had been at all in contact, during the time Murphy could be said to labour under the disease, it must have been in the evening of Saturday, Sept. 30, when Murphy was returning from Kensal Green, and may be supposed to have been already infected, although the symptoms had not declared themselves. But it is unlikely that a mere casual encounter in the street, if such occurred, could have infected the patient George. It is more probable that, if he derived the disease from Murphy, it must have been through the particles of poison floating over the neighbourhood. Even with this supposition, the period of incubation is rather prolonged (three to three and a half days). Such a supposition becomes probable in this case, only when its probability has been shown on other grounds.

**CASE X.** A daughter, aged 6, of the above patient (James George) was attacked on the same day (Oct. 4), and nearly at the same hour, with diarrhoea. At half-past eight she was conveyed to St. Bartholomew's Hospital, and admitted under Dr. Burrows, who considered it to be a decided though mild attack of Asiatic cholera. She ultimately recovered.

**CASE XI.** The eleventh case occurred in the same court, in Chelsea, in which the sixth case occurred, and in the person of the mother of that patient Susan Cook, aged 40, residing at No. 7, White Hart court, Duke street, Chelsea.

This patient was seen by Mr. Keen on the 4th October; at that time she presented symptoms similar to those of her son in an exalted degree. She continued to present nearly the same symptoms till the 6th, when she began to improve. She passed urine on the 7th, and faeculent motions on the 8th. She eventually recovered.

**CASE XII.** The twelfth case occurred in Horsleydown. A man, aged 77, residing in Horsleydown, was attacked at 5 a.m., Oct. 5, and died at 8 $\frac{1}{2}$  p.m., 15 $\frac{1}{2}$  hours after the first stool. Dr. Greenwood, who reported the case to the Board, had no doubt that it was a pure case of Asiatic cholera; and this opinion was shared by Mr. Bowie, who made inquiries into the case.

This patient lived in Horsleydown, and consequently may casually have been in contact with John Harnold (Case IV), who died between six and seven days before. I am not aware at what distance his dwelling was situated from the

lodging of John Harnold, but I believe it was not in the immediate neighbourhood. The reporters do not say a word as to any possible connexion between this case and that of John Harnold.

CASE XIII. The thirteenth case occurred in an altogether different locality, viz. on board the Dreadnought Hospital-ship, lying off Greenwich.

Robert Gordon, aged 16, a sailor, was admitted on board the Dreadnought, in the beginning of September, with the sequelæ of smallpox. A few days after his admission he was attacked with diarrhœa, and continued for about a fortnight to pass frequently rather copious frothy yellow stools. These, however, were gradually diminishing in number, and it was intended to discharge him from the hospital. On the morning of October 5, he was employed in assisting the nurses, and appeared as well as usual; but about the middle of the day the characteristic symptoms of Asiatic cholera presented themselves, and he died in about twenty-four hours.

It will be observed that this case occurred in a patient under treatment. It was not brought into the Dreadnought. By the kind permission of the officers of the Dreadnought, I took the opportunity of inspecting the admission-book, and learned that no sailor arriving in a ship from any port in or near which cholera was or had been prevalent had been admitted into the Dreadnought, for any complaint whatever, for some considerable time. The disease, therefore, could not have been brought on board by the clothes of some non-infected individual arriving from an infected ship.

CASE XIV. John Healey, aged 25, a cousin of John Murphy (Case IV), and living in the same room with him, was taken ill at 8 a.m., Oct. 5. This man was also a labourer in the gas-works, and out of employ. He was a very temperate man, but for some months had lived very badly, being sometimes thirty-six hours without food. He had never been subject to bowel complaint. He was first attacked on the morning of October 5; I saw him the next morning at 9 a.m., and he then presented all the signs of a man about to rally from a mild attack of cholera; he subsequently passed into the stage of consecutive fever, and died on the sixth day with coma and a black tongue.

CASE XV. Joseph Hill, aged 5, residing in White Hart court, Duke street, Chelsea, either next door or two doors removed from the house in which Cases VI and XI occurred. At 12, noon, Oct. 5, he was seized with vomiting, and when visited two hours subsequently, had passed into a state of deep collapse, cold and pulseless. He was not purged till a short time before death. He died at half-past 7 p.m. the same day.

CASE XVI. On the same day, Oct. 5, two cases occurred in Spitalfields, and are reported by Mr. Hart in the 'Lancet' (Oct. 14, 1848, p. 419). The subject of the first was Mary Ann C., aged 11, residing at Wilson's place, Spitalfields. She was suddenly attacked shortly after 8 o'clock, a.m., Oct. 5, and after being in a state of collapse for some hours, reaction came on, and she rapidly recovered.

CASE XVII. A sister of the above, aged 3 years, was attacked at the same time, and presented symptoms of a similar kind, but of less severity. On the 8th instant the mother of these two children (Case XXVIII) was attacked with undoubted cholera, and died in twenty-eight hours.

CASE XVIII. William Cook, aged 40, the father of Richard Cook, (Case VI) and the husband of Susan Cook (Case XI), residing at No. 7, White Hart court, Duke street, Chelsea, a man of drunken habits, had diarrhœa on the 6th October, having had occasional looseness during the previous week. At 6 o'clock p.m., October 6th, he was seized with genuine Asiatic cholera. He died at 2 a.m., on the 8th, having been ill about thirty-six hours.

CASE XIX. William Cook, aged 14, son of the above, residing in the same house, was seized at 6 p.m., October 6th, with vomiting and purging; and was seen at 8 a.m., October 7th, in the cold stage of Asiatic cholera. He died at 1½ p.m. On examination after death, the appearances usual in cholera were found.

**CASE XX.** Elizabeth Morris, aged 14, residing at No. 7, White Hart court, Duke street, Chelsea, in the same house as the family of Cook. She was attacked with diarrhoea on the 6th of October. This continued all day; at night she began to vomit rice-water fluid, and had cramps in the legs. When first seen at 8 a.m., October 7th, she was in the cold stage. She died at 1 a.m., October 8th, having been ill between thirty-six and forty-eight hours.

**CASE XXI.** John Rutherford, aged 47, convict on board the Hulk *Justitia*, lying off Woolwich; admitted into the Hospital-ship at midnight October 6th, in a state of decided collapse. He died at twenty minutes to 10, October 7th, having been ill thirteen hours.

**CASE XXII.** — Rabett, aged 30, a sailor, had been for several months a patient on board the *Dreadnought* Hospital-ship, with dysentery and pleurisy. He was, however, convalescent, and was about to be discharged at the time he was attacked with cholera, the first symptoms of which showed themselves on Friday night, October 6th. On the 9th reaction seemed to commence, but the bladder remained empty; he became drowsy, and this drowsiness deepened into coma. He then presented the usual symptoms of that form of consecutive fever which is complicated with non-elimination of urea, and died about eight days from the period of attack.

**CASE XXIII.** James Bigwood, aged 44, convict on board the *Justitia* Hulk, lying off Woolwich. Admitted at 3 a.m., October 8th, in the state of collapse; and died at 2½ p.m. on the same day.

**CASE XXIV.** Louisa Hill, aged 10, sister of Joseph Hill (Case XV), residing in White Hart court, Duke street, Chelsea. Attacked in the morning of October 8th. She ultimately recovered.

**CASE XXV.** James Paterson, middle-aged, a sailor, arrived from Shields on the 2d of October. Since then, had lived on board ship in the Pool. During October 7th, he suffered from watery diarrhoea, at 11 p.m. he had vomiting and cramps of moderate intensity. At 9 a.m., October 8th, when admitted into the *Dreadnought*, he presented all the characteristic symptoms of cholera, and died at 8 a.m., October 9th. It will be observed that this man had been five days in the Thames, and probably eight or ten days had elapsed since he left Shields, when the premonitory diarrhoea commenced. No other case had occurred on board the ship.

**CASE XXVI.** A man aged 44, residing at No. 7, Prescott street, Clapham Common. Although this case occurred after some of those which follow it, I give it here, as it is an interesting case.

On Sunday, October 8th, this man was as well as usual, and his wife stated that he had no bowel complaint. The patient himself, however, stated to Mr. Greenwood, of Clapham, who attended him, that he had suffered for two or three days before his death from relaxation of the bowels. His wife was not cognizant of this, and states that if the relaxation had been great she must have known it. On Monday, October 9th, he was as well or even better than usual. He went to his work about 9 a.m., and was employed for an hour and a half in emptying and repairing a drain, some distance from the house. The drain was a small one running from a sink. Another man opened the drain, and the subject of this Report carried the contents in a pail to the place where they were to be thrown. He had been accustomed to empty privies and drains, and did not on this occasion complain of any particular effluvia. On this day he took for dinner beef and bread, and for tea bread and butter. He took no supper. He was not purged during Monday night. He was quite well on Tuesday morning. The wife does not think that the bowels were at all relaxed. He made a very hearty breakfast, and went to his work. He came in at 1 o'clock to dinner, having been affected since 9 o'clock with painless diarrhoea. Soon afterwards the symptoms of collapse appeared, and he died between 9 and 10 the same evening, having been ill about twelve hours.

This case was one undoubtedly of Asiatic cholera; the nature of the symptoms

and the rapidity of their evolution seem to prove this; and in addition, the patient was visited by several of the medical men in the neighbourhood, who were unanimous in their diagnosis.

It becomes a question whether any predisposing or accessory influence is to be attributed to the effluvia from the drain, which the patient was engaged in emptying, on the day previous to his death. There was a doubt as to whether the patient really had suffered from any diarrhoea which might be considered premonitory, previous to this occurrence. If so, the effect of the drain would have been purely accessory, if not absolutely negative. On the other hand, if there had been no premonitory diarrhoea, and if these effluvia had a more active influence, of what nature was it? Did they render the frame of the individual obnoxious to the effect of the already present cholera-poison? Did they permit to be generated *de novo*, under the influence of some general occult atmospheric condition, the specific poison, which then happened to meet with an individual susceptible to its influence? To this notion there are many objections, of which I will only adduce three: 1. Such a development *de novo* has yet to be proved. 2. The development in so short a time (90 minutes) is unlikely. 3. If the specific poison could be formed so readily from drains under the influence of a peculiar choleraic atmosphere, cholera ought at this time to have been much more prevalent than it was, as in London there must have been many localities affording similar effluvia in as great abundance and concentration as those to which this patient was exposed. It is of course understood that the effluvia of the drain *per se* were quite incompetent to produce cholera,—a disease springing from a specific poison.

Altogether it seems most probable that the drain had no special effect, or that if it had any effect it must have been simply as an assisting accessory cause, posterior in point of time to the reception of the true choleraic poison into the body. At the same time there are still great difficulties about this case, which will probably remain unexplained till our knowledge of cholera is much greater than it is at present. The patient resided near Clapham Common, in a part of the town by no means overcrowded, not perfectly drained it is true, but still not very deficient in this respect; in a house not densely occupied, scrupulously clean, and dry; he lived tolerably well, and had not been exposed to weather. There was no cholera in the neighbourhood; he had not, as far as could be learnt, been out of the neighbourhood or been in contact with any infected person. In this neighbourhood also, even up to this time (Feb. 1849), there has been no second case of cholera, or at least none is known to the Board of Health. And yet this man, unsurrounded by the specific poison, unencompassed with the accessory causes which give that poison local power, died of malignant cholera in one of its severer forms. At least I do not see how the evidence to this effect can be resisted. Was there then some extraordinary susceptibility, some intense predisposition to the disease, which caused this man to fall a victim to a degree of intensity of poison quite inadequate to affect any second person, not so intensely liable? To this question it is at present impossible to give an answer; since we do not know the exact nature of the predisposition, nor, of course, the amount and extent of its influence.

The following cases, into the details of which I need not enter, but which were all unquestionable, complete the number which I consider sufficient for the present inquiry.

**CASE XXVII.** A sailor, named Rouse, was admitted from the Pool into the London Hospital, on the 8th Oct., with undoubted cholera. He passed through the cold stage, and died of consecutive fever.

**CASE XXVIII.** Mother of Mary Ann C., Spitalfields, Oct. 8th.

**CASE XXIX.** A man, named Thomas Baker, residing in Friendly place, South street, Long lane, Bermondsey, was attacked on the morning of Oct. 9th, and died at 2 a.m., Oct. 10th.

**CASE XXX.** David Davison, sailor, from Hartlepool, taken ill in the Pool, at 4 o'clock a.m. Oct. 9th; died in the Dreadnought at half-past 5 p.m. Oct. 9th.

CASE XXXI. A convict in the Justitia Hulk, Woolwich, admitted Oct. 10th; died Oct. 10th; seven hours and a half ill.

In addition to these cases, the following appear in the Registrar-General's Report. Cases of cholera are reported in August and September, but these were examples of common autumnal cholera—they did not exceed the average; and the Registrar-General states, in the Quarterly Report for the quarter ending September, 1848, that, "so far as the returns down to the end of September go, there is no trace of the epidemic of cholera in England." (p. 3.)

In the week ending September 30th, 4 cases of cholera were reported, the weekly average, as estimated from five years' returns, being 7.

In the week ending Oct. 7th, 13 cases of cholera were returned, the weekly average of five years for this week being only 1.

These 13 cases are thus stated in the Registrar-General's weekly Report:

"Fatal cases of cholera returned in the week ending Saturday 7th. (All are certified by medical attendant.)

"1. In Old street (subdistrict), St. Luke's, at No. 39, Rahere street, wife of a gentleman, æt. 59, 'disease of the bowels, simulating Asiatic cholera' (38 hours' duration).

"2. In south subdistrict, West London, F. 27 years, 'cholera' (13 hours' duration).

"3. In St. Bartholomew's Hospital, West London, M., about 40 years, 'Asiatic cholera.'

"4. In town (subdistrict), Bethnal Green, at No. 4, Cheshire street, a weaver, F., 21 years, 'enlargement of the heart' (12 months' duration), cholera spasmodic (12 hours).

"5. In Spitalfields, Whitechapel, M., 23 years, 'cholera' (12 hours' duration).

"6. In Whitechapel, north, a girl, 4 years, 'English sporadic cholera' (7 days' duration).

"7. In St. Paul (subdistrict), St. George's in the East, M., 38 years, 'cholera' (2 days' duration).

"8. In Mile End Old Town, Lower Stepney, M., 47 years, 'cholera' (36 hours' duration).

"9. In Lambeth Church, second part (subdistrict), at Orsett street, daughter of a chairmaker, 11 months, 'cholera' (3 days' duration), convulsions (1 hour).

"10. In Rotherhithe, a boy, 11 years, 'cholera' (19 hours' duration).

"11. In same subdistrict, F., 38 years, 'cholera' (19 hours' duration).

"12. In same subdistrict, a girl, 2 years, 'cholera' (2 days' duration).

"13. In Greenwich, west (subdistrict), F., 37 years, 'cholera' (4 days' duration)."

Of these cases, two (Nos. 2 and 3) appear among my list; eleven are distinct cases. It is quite uncertain how many of these were examples of Asiatic cholera; choosing, however, the severest cases, we may suppose four (viz. Nos. 5, 8, 10, 11) to have been of this type. Some of the others may have been so, but of course this is still more doubtful. We shall presently see that their omission will not seriously prejudice either argument. I may remark, that the cases I have given, or rather those of them which were fatal, are registered among the later weekly returns of the Registrar-General.

I believe that no one who examines the evidence for himself, will have any difficulty in admitting, that the range of cases now reported, commencing with that of John Harnold (Case IV), were all examples of true cholera.

Taking this for granted, the accompanying table will show the next point in the inquiry. The table is divided into four columns:—in the first is the date of attack; in the second, the name of the individual; in the third, the locality he inhabited; and in the fourth, the infected person, if any, with whom he had been in contact or proximity.

In contact or not with a person labouring under cholera.

Date.	Name.	Locality.	In contact or not with a person labouring under cholera.
Sept. 28	John Harnold	Horsleydown	{ With the mate of his vessel, who was said to have died of cholera—case not certain; with cholera patients in Hamburgh?
Sept. 30	{ John Murphy Richard Cook	Lambeth	Not in contact with any cholera patient
Oct. 1	Jane Langham	Chelsea	Not in contact with any cholera patient
Oct. 2	Owen Jones	Harp court, Fleet street	Not in contact with any cholera patient
	{ James George	Justitia Hulk, Woolwich	Not in contact with any cholera patient
Oct. 4	Daughter of above	Lambeth	Not known to be in contact with any cholera patient
	Susan Cook	Lambeth	In contact with her father
	An old man	Chelsea	In contact with Richard Cook
	Robert Gordon	Horsleydown	Not known to be in contact with any cholera patient
Oct. 5	{ John Healey Joseph Hill	Dreadnought, Greenwich	Not in contact with any cholera patient
	Mary Ann C.	Lambeth	In contact with John Murphy
	Sister of above	Chelsea	Probably in contact with Richard and Susan Cook
	William Cook	Spitalfields	Not in contact with any cholera patient
	Son of above	Spitalfields	With her sister
Oct. 6	Elizabeth Morris	Chelsea	In contact with Susan Cook
	John Rutherford	Chelsea	In contact with his father and mother
Oct. 7	— Rabett	Chelsea	With the above
	James Bigwood	Justitia, Woolwich	With Owen Jones
	Louisa Hill	Dreadnought, Greenwich	With Robert Gordon
	James Paterson	Justitia, Woolwich	With Owen Jones and John Rutherford
Oct. 8	Rouse (a sailor)	Chelsea	With Joseph Hill
	Mother of Mary Ann C.	Pool	Not known to be in contact with any cholera patient
	James Baker	Spitalfields	With her daughters
	David Davison	Bermondsey	Not known to be in contact with any cholera patient
Oct. 9	A convict	Pool	With the convicts mentioned above
	A man, aged 44	Justitia, Woolwich	Not in contact with any cholera patient
		Prescott street, Clapham	

It thus appears, that, from the 28th September to the 10th October (twelve days), 28 cases of cholera occurred in ten different localities, situated, not in proximity, but in various and remote divisions of this immense city.

The inhabitants of these localities do not appear to have held any intercourse with each other; and in no single instance could it be discovered that the *first* person sick in any particular locality had been in contact or proximity with another individual previously diseased, in another locality.

The following considerations support this assertion :

1. Such contact was in no case alleged to have taken place by the patients themselves, nor by their relations. John Harnold had been in London only three days before his death, and it is not known how he was employed during those three days; but most probably he was at the docks. John Murphy, the second case, resided some miles away from Horsleydown or the docks; and, although I did not specially inquire whether he had been at the docks (being ignorant at the time of the case of John Harnold), I made general inquiries into his habits, and learnt that he remained usually at home, but, on the day preceding his death, had gone to Kensal Green, a district situated five or six miles on the Harrow road, in a contrary direction to Horsleydown or the docks. Jane Langham, again, lived near Fleet street, in a place far removed from either Lambeth or Horsleydown; and in her case it was proved, that she had not been out of the immediate neighbourhood for some time before her death. In respect to the cases at Chelsea, again, I begged Mr. Keen, who attended them, to inform me if any facts had come to his knowledge, which could lead to the suspicion that his first cases had been in contact or proximity with any diseased persons; Mr. Keen replied, that no such communication was traceable. What, indeed, except accident, could have brought John Murphy, the labourer in the gas-works at Lambeth, in contact with Harnold, the seaman of Horsleydown, or either of these with the wife of a coalporter in Fleet street, with a convict in the hulks at Woolwich, or with a sick man on board the Dreadnought Hospital-ship at Greenwich?

2. But if these several persons were in no way connected by trade, profession, or calling, if they were not exposed by proximity of habitation to the emanations emitted from the bodies of each, is it not possible that some extraordinary series of accidents may have brought them together; and that, without actual knowledge of the fact, John Murphy may have met John Harnold, Jane Langham, or the boy at Chelsea, and have infected, or have been infected by these persons?

To those who know the localities above named, and who are acquainted with this vast metropolis, this question will appear almost absurd. To those who are not possessed of this information, it may be mentioned that Horsleydown lies on the south side of the Thames, to the eastward of Bermondsey, and is situated close to the river, at a point nearly opposite to that where St. Katharine's docks are placed. From Horsleydown to Lambeth, by the river, is a distance of several miles, as the Thames here makes a large curve. As the crow flies, however, Horsleydown is about two miles distant from Lambeth, or rather from Lower Fore street, where the second case of cholera occurred. The two spots are separated by the districts of Bermondsey and Southwark, which compose an immense city, crowded with inhabitants. The spot in Chelsea, in which the next case occurred, is on the other side of the river, and close to Battersea Bridge; it is, in a direct line, about two miles from Lower Fore street, Lambeth, and four from Horsleydown. Harp court, Fleet street, is in the city of London, about half way between Temple Bar and Ludgate Hill, and is separated from the Thames by the breadth of Fleet street, and the length of the lanes running from Fleet street to the river. As the crow flies, it is about two miles, or one mile and a half from both Horsleydown and Lambeth. It is about four miles from Chelsea. Woolwich is situated about eight miles from Horsleydown, ten from Lambeth and Fleet street; and twelve from Battersea Bridge. The

Dreadnought Hospital-ship lies off Greenwich, about three miles above the Justitia, and five from Horsleydown. Spitalfields, Clapham Common, and Bermondsey are also districts widely separated from each other. The spot in Bermondsey, in which the case occurred on the 9th of October (eleven days after John Harnold's case), was at the part of Bermondsey most remote from Horsleydown. If the four cases which occurred in the Registrar-General's weekly return, of the week ending October 7, are included, the following localities will have to be added, Stepney (one case) and Rotherhithe (two cases), both places being some considerable distance from the localities given above. The fourth case occurred in Spitalfields. The localities would thus be increased to twelve; the cases to thirty-two. As far as can be known, therefore, to leave out these four cases, and the two additional localities, is not likely to prejudice the strict contagious argument; whilst any addition to the number of localities would, *cæteris paribus*, be unfavorable to it.

Now as these twenty-eight were the only cases of cholera occurring in London within ten days (with the exception of the Registrar-General's four cases, which, if included, would even increase the number of affected localities), if we seek to derive the disease of each patient from contact or proximity with a previous case, we must have recourse to the most extraordinary suppositions. We must suppose that John Murphy had somewhere, in the densely crowded streets of London, brushed past John Harnold (who had been only three days in town, and was probably at the docks all day, rather than in the neighbourhood of Lambeth); that Jane Langham, in the same extraordinary way, also had a fatal rencontre with one of these two individuals; and that in a similar manner all the first cases in the several localities must have obtained the disease, by this accidental passing in the street of an individual whose residence was many miles away.

But the probability of such accidental encounters in London will appear to every one to be unsustainable for a moment; and, in addition, is it possible that the rapid transit of individuals should in this way communicate cholera? If so, cholera is the most virulent of contagions, and nothing could prevent it from half depopulating entire London. But if this rapid transit did communicate it, it must be remembered that the only time John Murphy could have met John Harnold, or Richard Cook could have met either of these two, or Jane Langham any of the three, must have been before the symptoms were developed, even in the shape of premonitory diarrhoea, which symptom, indeed, did not occur in the first two cases, and commenced at night in the third. But I need not delay on this topic, as the next argument is absolutely conclusive against the opinion in question.

3d. In two instances, if not in more, it is quite certain that there *could* not have been even an accidental encounter between these patients and others labouring under cholera. In the hulk at Woolwich, the first case occurred on the 2d of October. Now the convicts at Woolwich may be said to be in a kind of quarantine; they work in the dockyard, are watched by the appointed officers, and are allowed no intercourse with other persons. The Justitia Hulk was lying about three miles below Greenwich, and far apart from any other vessel, except the convict hospital-ship. No merchant vessel, indeed, ever anchors at this point of the river; or if any vessel should anchor, it is merely on account of the tide, and the place of anchorage would be many hundred yards from the spot where the Justitia was moored, which was close in shore; so that if cholera had been raging in Woolwich, and had been prevailing in the vessels in the Thames above Woolwich, the origin of cholera in the Justitia would still, in all probability, not have been attributed to contagion. But there was no cholera in Woolwich, or in the merchant vessels in the Thames; and the only cases in London which were anterior in point of time to this at Woolwich, were those of John Harnold, at Horsleydown (seven or eight miles distant), John Murphy, at Lambeth (twelve or thirteen miles distant), Richard Cook, at Chelsea (thirteen or fourteen miles distant), and Jane Langham, in Fleet street,

(ten or twelve miles distant). The occurrence, then, of anything like contact or proximity between these individuals and Owen Jones, the convict at Woolwich, may be said to have been absolutely impossible. So again in the Dreadnought Hospital-ship, Robert Gordon was attacked on the 5th of October. The Dreadnought lies off Greenwich, and is three or four miles away from the Justitia, with which it holds no kind of communication. It is also many miles removed from Horsleydown, Lambeth, Chelsea, and Fleet street. Robert Gordon had been for a month on board before his seizure; he could not, therefore, have been in contact or proximity with any of the nine cases which occurred previous to his attack.

A question may be put here in respect to these two cases, which I shall have to put presently to all the cases. Although Owen Jones and Robert Gordon could not have been in contact or proximity with any of the known previous cases, may they not have been exposed to some other source of contagion?

To this it may be answered—

(a) That none such is known. On board the Justitia, as already said, the convicts were far removed from all other vessels, and were not allowed intercourse with any one. And on board the Dreadnought, as I have stated in a former part of this Report, no sailor arriving from any infected place, had been admitted with *any complaint whatever*, for some considerable time prior to the case of Robert Gordon. The Dreadnought also lies in a clear part of the river, and is several miles below the Pool where the merchant vessels lie.

(b) No other source of contagion can be indicated, except the cases above given, and unreported cases which may have occurred among the merchant vessels in the Pool. For these London cases are among the earliest in the kingdom, being only four or five days posterior to the cases at Hull; so that the number of possible cases is reduced to so small a number, that we can feel certain that nearly all chances of error arising from unreported cases are avoided.

It may therefore be concluded, with as much certainty as can ever be obtained from evidence of this kind, that—1, as there is no evidence of the persons mentioned above having been in contact or proximity with each other;—but on the contrary; 2, as such contact or proximity is, from the respective nature of the localities in which these cases occurred, and from the nature of the occupations in which the individuals were engaged, in the highest degree improbable; and, 3, as in two cases it is *absolutely* impossible that there should have been such contact or proximity;—therefore, the strict contagious theory, viz. that which supposes that an individual derives the epidemic disease from being in contact or proximity with a person already sick of the same malady, is not capable of explaining the mode of origin of the earliest cases of cholera in London.

The argument on which this conclusion is founded can be equally supported, if any of the cases now considered as cholera are denied to be of this nature. It is not worth while to go over the steps of the problem; but if the cases of Richard Cook, Jane Langham, or any others, are considered doubtful, and are left out, other cases become available, by which it is still more clearly proved that contact or proximity must have been impossible; and in addition, the period between the successive attacks becomes too great for the ordinary period of incubation, so that an additional argument arises against contagion.

We may ask here, whether any of these parties could have been exposed to any other “focus” of poison? I have already given reasons for believing that this could not have been the case with the cholera patients of the Justitia and the Dreadnought. There is no evidence of such latent or unknown source of contagion in the other cases. John Murphy had been for months resident in Lambeth; no other case had occurred at that time in Lambeth.\* The family of Cook had been living in the same house in Chelsea in which they were at-

\* I think Mr. Wagstaffe reported a suspicious case; but this was several months previously.

tacked ; no other person had had cholera in Chelsea since Mr. Haden's case, four months before. Mary Ann C. and her mother and sister lived in Spitalfields. No other case had occurred in Spitalfields at that time (unless the Registrar-General's case was anterior). James Baker was the first case in Bermondsey. The man in Prescott street, Clapham, was the first and only case in that locality, and had not been in contact with any diseased person. In fact it is needless to dilate on this point, as the chances are inconceivably small that all these persons should have been exposed to the influence of cases of cholera which are unknown to us. For it must be remembered that cholera is a disease so frequently fatal, and marked by symptoms so characteristic, that it is not likely any case would have been overlooked by the practitioner who attended it, or would not have been included, if fatal, in the Registrar-General's returns.

But now, as the *strict* contagious theory has been shown to be inadequate to explain these cases, can any modification of it explain them?—such as that which alleges that it is erroneous to limit the extension of the poison to a few yards, but that the particles of virus passing off from an infected person may float hither and thither to an indefinite distance, so that persons miles distant from the affected individual may really derive their disease from him.

If this modification of the original contagious doctrine be kept perfectly consistent with its parent theory, which maintains that the only multiplying source is the human body, it must on no account be admitted that particles of poison emitted from a diseased person can be increased in number after they leave the body. The particles are merely so far changed, when they float into the air, by the actual condition of that air itself; they may become more or less volatile, more or less stable, according to its heat, its humidity, or its electrical condition; or they may be wafted to a greater or less distance by the force of winds, or by the direction of currents of air. But they do not increase in number until they have entered another human body, and find in it the means of generating a fresh supply of poisonous particles. If this limitation be not kept in view; if it be admitted that particles given off from a diseased person may actually generate—external to the body—other particles like themselves; then of course the question is entirely different, and the relative power of the human body and of other conditions is to be settled by evidence brought to bear on the point.

But now let me inquire whether the following hypothesis can be supported. Let us put aside all the cases previous to that of John Harnold. Let us suppose that this man derived the disease either directly from cholera patients at Hamburg, or from the sailor who died at sea. Let us suppose that Harnold entered London an infected person, and that three days subsequently, when the disease declared itself, particles of cholera poison floated off from his body into the surrounding atmosphere. Is it then possible that these particles should have drifted to Lambeth, to Woolwich, to Fleet street, and to Chelsea; and then on to other localities in which the disease appeared, and which may then have been also infected by the particles given off by these secondary foci, which also floated into the air, and augmented the number of those emitted by the first and imported case?

Such an hypothesis as this is neither supported, nor is it absolutely controverted, by direct evidence. But there are strong reasons for believing that it is not tenable. Of these reasons the following are the most stringent:

1. The above modification of the contagious theory necessarily assumes that the specific poisons derived from the animal body are not mere chemical or inorganic gases, because (putting other arguments aside) if they were, they would diffuse themselves rapidly according to the usual law of gases, and would doubtless be so speedily diluted as to make it hardly conceivable that the atmosphere should be dangerously tainted by them, over any great extent.

But if not gases, these poisons must be supposed to be organic particles, not endowed with life, or with means of multiplication. They must be particles thrown out with the excretions, with the carbonic acid from the lungs, with the perspi-

ration from the skin, or with the excreta from the urinary or the intestinal surfaces.

But if this were the case, it is highly improbable that these particles would not, immediately after their entrance into the atmosphere, undergo the usual chemical changes to which organic matters are always subjected.

"The presence of the odoriferous principles of plants, the miasmata of marshes, and other matters of contagion," writes one of the most profound philosophers of our time, "although sufficiently obvious to the sense of smell, or by their effects upon the human constitution, cannot be detected by chemical tests. But it may be remarked in regard to them, that few or none of the compound volatile bodies we perceive entering the atmosphere, could long escape destruction from oxidation. The atmosphere contains, indeed, within itself, the means of its own purification, and slowly but certainly converts all organic substances exposed to it into simpler forms of matter, such as water, carbonic acid, nitric acid, and ammonia."\*

If, therefore, such organic particles undergo these changes, we can hardly suppose it likely that they will thus drift in their original intensity through several hundred thousand feet of atmospheric air,—as must have been the case in the present instance, if they reached Lambeth and Chelsea from Horsleydown.

2. But whether the specific agent be an inorganic gas, or a cloud of organic particles, it is to be expected that, if given off in the manner supposed in the hypothesis, it would produce its greatest effects while most concentrated, viz. in the vicinity of the patient from whose body it was emitted. But this was so far from being the case in the present instance, that the agent must have passed innocuously over the tens of thousands of susceptible persons crowded in the dense lanes and alleys of Horsleydown, and have actually struck down its first victim at a spot separated from its source by myriads of houses and an immense space of ground; its second at a spot yet more remote; and its two next in localities still farther removed from all these places, and very distant from each other. If, indeed, the next cases had occurred in Horsleydown within two, three, or four hundred yards from the house where John Harnold died, we might have conceived the possibility of this driftage of poisonous particles; but it is surely an extreme assumption to extend this distance to an indefinite extent, more particularly when, as in the present case, there is no evidence of any cases occurring for some time subsequently between the two points of this extensive range.

3. There is another argument against such a view. There is strong reason to believe that the cholera-poison is not a very volatile one; it often limits itself in an extraordinary manner to one locality in a town; a street or narrow river sometimes separates a region in which the disease is raging severely, from one in which it finds no victims. And it must be understood that the class of persons in the affected and in the non-affected district is the same. Therefore it must surely be erroneous to note in one case the absence of volatility, and the next moment to ascribe to the poison so remarkable a volatility, that it shall drift for many miles in different directions, north, south, and east, as must have been the case, if the poison given off from John Harnold excited the disease in Murphy, Cook, Langham, or Jones.

We can hardly suppose, too, if the particles of poison were thus at the same time volatile and stable, that when three or four persons had been affected and contributed their quota of particles to the atmosphere, the multiplication of cases would have gone on in a progressive ratio, but this was not the case.

There are other arguments against this view, drawn from the direct evidence in favour of other modes of origin; but to these I do not wish now to allude.

If neither the strict contagious theory, nor this derivative opinion, can satisfactorily account to us for the production of these first cases in London, shall we

\* Elements of Chemistry. By Thomas Graham, F.R.S., &c. &c. Second edition, p. 336.

derive more satisfaction from the opinions of the party who have been termed, though as I think erroneously, non-contagionists?

This opinion of this party is distinguished by the fact that, without denying to the human body a power of reproduction of each specific poison in various degrees according to the poison, or perhaps also to the condition of the body, it yet contends that the poisons can increase when altogether disconnected from the human system.

In the case of each poison, this party seeks to learn what are the circumstances most favorable for this increase external to the living organism; and these circumstances (the so-called conditions of increase and existence) are determined by actual observation of the predilection of the individual poison which may be under consideration, for special sites and localities, or for particular classes of men.

As yet, the local conditions which appear to be the most favorable for the propagation of the cholera-poison are alleged to be the following: 1. A low, damp locality, particularly one situated on the banks of rivers, or on marshy ground. 2. A close, impure air, from the accumulation of individuals, and from the effluvia arising from them. 3. Any circumstances which may cause organic particles to be continually emitted into the air, and which form over the locality a kind of canopy, which is continually dissipating itself into the surrounding purer air, and is as continually renewed by exhalations from below.

These conditions being present, it is alleged, from various reasons, that the cholera-poison increases and augments itself when once it is present in the locality.

It may become present in such a locality in four conceivable ways:

1. By the arrival of an infected person, whose body throws off the particles of poison, which then meet in the atmosphere around them with the conditions of increase.
2. By the arrival of one or more non-infected persons, to whose clothes or baggage particles of poison may have adhered during their passage through an infected district.
3. By the introduction of the poison, either by currents of air or from the influence of some peculiar force,\* which causes the cholera-poison to pass over tracts of country in determinate directions.
4. By an actual generation of poison in the locality itself; a peculiar atmospheric or other dynamic state having concurred to make up, with the previously existing conditions, the sum of those circumstances whose assemblage causes the generation of the particular poison proper to that conflux of conditions.

It may be believed by this party that the cholera-poison can travel in all these methods, or may be manifested in one spot after another, in consequence now of one, and now of another, of these modes of propagation. The relative frequency of any of these methods, or their actual occurrence, it may leave to be settled by evidence; but as a general rule, it may believe that the degree of the several local conditions (the state of the receiving bodies remaining constant) will determine the prevalence of the disease in one locality rather than in another, and that by taking these conditions into account, the places where cholera will prevail most severely, supposing the poison to have once entered, may be pointed out. At the same time, in countries in which cholera is not indigenous, in addition to these local conditions, a general atmospheric state (the nature of which is as yet unrecognised) may be assumed; and in the countries in which cholera always more or less prevails, an increased prevalence of this choleraic

\* The existence of this force must be admitted in the case of influenza, whose course has a singular relation to that of cholera. What this force may be—whether (for instance) it is allied to electricity in some form—cannot at present be known. May it not be something which, even yet, we have not fathomed? Who can yet say, with the wonderful discoveries in magnetism which the genius of Faraday or Oersted discloses, that some other allied or separate forces have not yet to be recognised in Nature?

constitution may be supposed occasionally to occur. If this view turn out to be incorrect, it is yet alleged that it hypothetically expresses better than any other opinion the connexion of the phenomena under review. Into the proofs and general considerations connected with these subjects I do not wish now to enter, but simply to consider what support is given to any particular opinion by the early cases in London.

In twelve days, 28 cases occurred in ten different localities. In six of these localities, the circumstances which have been asserted to be efficient in aiding the activity of the cholera-poison were present in a high degree; in three others they were also present; but I have only general facts to bear on the subject, no special inquiries having been made. In the tenth locality, viz. in Prescott street, Clapham Common, in which a single case occurred, the conditions of development of the poison did not exist in a marked degree. The origin of this case, as stated in the account of the case itself, is rather obscure.

The six localities in which moisture, effluvia, and impure air were abundantly present were:—1, Lower Fore street, Lambeth; 2, White Horse court, Chelsea; 3, Harp court, Fleet street; 4, Hulk Justitia, Woolwich; 5, Spitalfields; 6, the Pool (part of the Thames).

In three other localities, viz. in Bermondsey, Horsleydown, and on board the Dreadnought, the poison must have met equally with moisture and effluvia. The case at Bermondsey occurred in the immediate vicinity of Long lane, which has long been notorious for its open ditch, its crowded and miserable houses, and for its general wretched sanitary condition. The cases at Horsleydown were also in a wretched neighbourhood close to the river; and on board the Dreadnought, although the ship is scrupulously clean, yet of course it is exposed to the effluvia from the numerous patients it contains, from the bank of the Thames near which it lies, and from the surface of the tainted water.

The point being conceded that in nine localities the poison would have met with favorable conditions of development, we have to inquire how the poison arrived in these localities;—was it brought there? did it travel there? or was it generated there?

The scrutiny to which these cases have been submitted, proves that in the greater number of the localities—in all, in fact, except Horsleydown—the poison was not imported by any person ill of cholera arriving in the locality. Nor could it have been brought by the clothes or baggage of any persons coming from infected districts in England, as there were no infected districts from which such persons could have come. There is no evidence either of any persons arriving from the Continent, from Hamburg or from Dantzig, to these localities. If such persons did arrive in London, it is most probable that they were connected with the shipping, and were therefore many miles removed from Lambeth, Chelsea, and the other places in question.

In Horsleydown, however, the poison may have been imported; but if so, it is a very interesting point that it did not seem to propagate itself by contagion or otherwise. The next case which occurred in this locality was seven days after that of John Harnold, and after this there were only a few scattered cases for many weeks. The poison therefore died away, or rather remained at its lowest point of intensity.\*

If therefore the poison was not brought to Lambeth, Chelsea, the hulk Justitia, or the Dreadnought, by infected persons or non-infected persons or clothes, did it enter in some other way, or was it generated in these localities?

*Per viam exclusionis*, it is necessary to admit either of these modes rather than the two former. It is not unreasonable to suppose that some moving force, altogether

\* Although it so happened, that the first case in London, omitting the anterior sporadic cases, was in the person of a man who perhaps brought the disease with him, I think all the facts seem to prove, that the almost simultaneous appearance of other cases was a mere coincidence. I do not see how it can be otherwise; and yet, in the history of cholera, there are a good many of these coincidences.

independent of the bodies of men, may have driven particles of cholera-poison into these localities ; for certainly there has been some power which, independent of human intercourse, and in opposition occasionally to winds, has driven this disease from the steppes of Tartary to the English shores.

With regard to the generation of the poison, there is no doubt that in Horsleydown, as well as in Lambeth, Chelsea, and in many parts of London, some of those circumstances which we have termed "local conditions of existence" existed in considerable intensity, and yet the cholera-poison did not develop itself in any commensurate degree. Was there not some essential condition wanting, whose deficiency prevented or nullified the effect which the other conditions would have had if conjoined with it ? And if so, what was this condition ?

And it may be remarked that this evident inability to propagate itself rapidly\* was displayed in all the localities—at Chelsea, at Lambeth, and even at Woolwich ; and it has continued to be marked, more or less, even to the present date (February).

At Chelsea, although the court in question was close to the river, was in a most filthy state, and was crowded with inhabitants, only six cases occurred ; the disease then stopped, and has not since returned. In Lower Fore street, Lambeth, although a place which had actually been pointed out by Mr. Wagstaffe before the cholera appeared in England, as eminently displaying the local conditions favorable for the spread of the disease, only four cases occurred, and then the disease stopped for a month. And even on board the *Justitia*, although cases continued to be furnished until the convicts were moved from their unhealthy anchorage, yet they occurred slowly, and with intermissions of days, or occasionally even of a week, between each case.

I confess that I was much struck with this peculiarity, since in India (at any rate of late years) if in any large town presenting conditions like those of London, cases of cholera had been seen to occur simultaneously in several districts, the observer might almost certainly have predicted that that town was about to suffer from a general epidemic. The way in which the cholera almost died away out of London, is not explicable on the idea that the sanitary condition was too good to furnish the poison its local conditions of increase, nor is it explicable on the contagious theory, or on any modification of it.

The previous hypothesis of a choleraic constitution seemed to me the only likely way of accounting for the non-development of the poison after its introduction. When it is considered how cholera has lately passed like a broad belt over a particular part of Europe, leaving untouched the districts on either side, although these districts were populous, held free intercourse with infected districts, and presented hygienic conditions favorable for the reception of the poison, we shall see cause to believe that there is some element yet unindicated, connected with the actual progress of the disease,† which is wanted to complete the sum of conditions under which the poison attains its highest

\* This also tells against the strict contagious theory ; as, if susceptible human bodies were all that the poison wanted, these have of course been abundantly supplied to it.

† As an illustration of this point, I may refer to the 'British and Foreign Medico-Chirurgical Review,' Jan. 1849, p. 8, which is taken from the description, given by M. Leseque (*Archives Générales*, Sept. 1848), of the course of the cholera in Russia.

" The peculiar force which impelled the cholera thus towards the north, is not only indicated by the general course of the disease, but by a closer study of its peculiarities. Whenever the malady deviated, so to speak, from its normal direction, and passed towards the west, it seemed incapable of propagating itself, and died away spontaneously, even in places which appeared to be well fitted for its reception. If, on its route, it encountered a large and densely-peopled town, it arrested itself there by preference ; but the existence of a similar town out of the line of its course did not seem to be able to attract it, or to cause it to wander from its singular march. The exemption of the provinces to the east of the Volga might be perhaps explained by the nature of the country, in which the inhabitants are few and scattered ; but the rich, fertile, and densely-peopled countries to the right of the Dnieper enjoyed an equal freedom from attack, which can only be explained by the fact, that they were situated beyond the line of the disease, which, as already stated, was principally between these

development. May not this unindicated element be a peculiar, as yet, unrecognised atmospheric condition which accompanies the poison, or which even, by meeting with certain local conditions, develops it? In London it may be supposed that this atmospheric condition has not yet been perfectly established, but that having been partially developed in the remarkable weather which ushered in the month of October, it has remained ever since stationary, or but slowly advancing to a more complete manifestation. In all these northern countries, to which the cholera-poison is foreign, it may be supposed that a particular atmospheric state is necessary.

Leaving, however, this conjecture—for at present it is little more—I may remark, in conclusion, that I have desired merely in this Report to apply the strict contagious theory to these early cases. I have decided that this theory cannot explain them. I have done so not merely on negative evidence that no contact or proximity between these early cases could be traced, but on positive testimony that such contact or proximity was impossible. These cases are, then, to be accounted for on other grounds; into these, however, I shall not at present enter more fully: contenting myself with the remark, that it appears to me more probable that the cholera-poison should have entered the localities first infected by virtue of some peculiar force acting irregularly and partially, so as to drive the poison into one rather than into another locality;—or that the poison should have been actually generated in those localities under the influence of a general atmospheric condition co-operating with the local conditions proper to each place;—than that it should have reached those localities by direct emission from diseased individuals in the vicinity or at a distance, or from clothes which had been in contact with infected persons.

I feel, however, that, without enlarging my basis of inquiry, I have no right to push this argument farther. To do so with any effect, facts drawn from a more extensive series of observations would have to be passed in review.

Whether or not the evidence which has satisfied my own mind will prove as satisfactory to others, I do not know; but I can affirm that I have collected this evidence with impartiality, and have stated it with sincerity.

#### APPENDIX.

As far as we can at present judge from what is known regarding the spread of cholera in Great Britain, there appears to be much evidence in favour of the position that this extension has occurred in two ways, most commonly by independent manifestation in particular localities, but sometimes by transmission through the medium of diseased persons. Some excellent Reports published in the 'Edinburgh Monthly Journal,' indicate that in Scotland in some cases importation was improbable; but that, in a few cases, the disease appeared to arise from importation, and did really manifest contagious properties. In London, while I have no doubt that contagion had no influence in originating the disease, the cases arising after contact with the Tooting children seem to prove the fact of occasional contagion; as do also the cases recently put on record by Dr. McWilliam (Med. Gaz., June 15). If we are not disposed hastily to reject all the evidence on one side, and as hastily to receive all the evidence on the other, I do not see what conclusion can be arrived at, but that cholera is occasionally, but only exceptionally, contagious.

In the April Number of the 'American Journal of Medical Sciences,' is some interesting information respecting the appearance of cholera in New York and New Orleans. On the 9th of November, 1848, the packet ship New York left Havre, with 352 emigrants and 33 crew, for New York. There had not been a

rivers. Although communication was not interrupted, as the system of cordons sanitaires was abandoned by the government, no effect was produced by this cause. The most frequent intercourse with the dense population of the west, living under very unfavorable hygienic conditions, even the lowering of the temperature, which, it might be supposed, would have caused the extension of the disease towards more temperate climates, produced no effect."

single case of cholera at Havre when they sailed. They had no case on board till the 25th of November, fifteen days after leaving the land, when a young German, in robust health, was attacked with vomiting, purging, and cramps of the extremities ; he died on the third day. On the following day, the 26th, another case occurred, and proved fatal on the second day. On the 27th, a girl died in two hours with the same symptoms. On the following day, a boy died in four hours and a half. On the 29th, a man died in seven hours. On the 30th, two more children died. On the 1st of December the ship came to an anchor at the quarantine station, Staten Island, and before the passengers landed on the 3d, twelve more cases had occurred. At the time there was no cholera on Staten Island. The sick emigrants were sent to an hospital—the healthy to some large public stores, in which were about seventy persons convalescent from other diseases. A person who had been treated at Staten Island for fracture of the patella, assisted in the removal of the sick ; three days subsequently, he was attacked with violent symptoms of cholera, and died the same day. Another man, who had been in proximity with the emigrants at the stores, left Staten Island on the day after they landed and went to New York ; he was attacked with cholera, was sent back to the quarantine ground, and died on the same day as the former case. Three other persons, who had been resident on Staten Island, and in proximity with the emigrants, were subsequently attacked. It does not appear that these last four cases had been in contact with the sick particularly ; but they were the occupants of the storehouse to which those of the emigrants who were not affected with cholera had been sent. Two more persons were afterwards affected ; making eight in all. No persons were attacked except those mixed up with the supposed healthy emigrants at the stores ; 43 of whom appear to have been afterwards attacked. None of the crew were attacked. Among the emigrants, all the victims were Germans ; of whom there were 270, all of them *long resident* in or near Havre. The disease then died away, and did not appear again either at the quarantine station, or at New York itself.

In this case there seems no doubt but that seven persons became affected with cholera after being in contact with a body of persons, some of whose number had suffered, and others were about to suffer from this disease. A still more remarkable fact is the origin of this attack at sea, fifteen days after leaving the land. It can hardly be supposed that it was derived from Havre, where cholera did not prevail ; this supposition also necessitating the allowance of an unusually long incubative period. It is perhaps more probable to suppose it to have been actually generated in the ship, which, no doubt, furnished unhealthy hygienic conditions in abundance, under the influence of which, and of some peculiar epidemic constitution, the special cause may have been brought into being.

The second case referred to in the 'American Journal' is somewhat similar to this. It is given by Dr. Fenner.

On the 11th December, 1848, the ship Swanton, 39 days out from Havre, arrived at New Orleans with 280 German and French emigrants. About ten days previously, several cases of bilious cholera had occurred in New Orleans, which all recovered, and were probably unconnected with the subsequent epidemic. When the Swanton left Havre on the 2d or 3d of November, there was no cholera in that place. The vessel was out twenty-six days before any death occurred ; the first was from consumption, afterwards sixteen or seventeen deaths took place, mostly from bowel-complaints, supposed to be dysentery. On the 12th of December, a woman from the vessel was taken to the Charity Hospital with undoubted cholera. On the 13th, another emigrant was admitted, and also died. *On the same day three other cases of cholera were admitted, all of which proved fatal ; none of these were passengers of the Swanton, and they were from different parts of the city.* On the evening of the same day, December 13th, another suspicious case occurred in a resident, who had not been near the ship Swanton nor seen any of the passengers. On the 15th of December, 8 cases were admitted into the Charity Hospital, and several occurred in private practice. On the 16th,

11 cases were admitted, and the cases in private practice were rapidly increasing in number. By the 22d, forty-five deaths had occurred in the city, and after this the epidemic rapidly extended. It was stated that the first three victims were cooks, who went every morning to the principal market, within a cable's length of the vessel; but this was erroneous, as the Swanton was nearly a mile away, and these cases occurred on the 15th, after several other cases had been noted. The disease reached its zenith about the 28th, and declined steadily from the 1st of January.

After the disease appeared, almost every vessel that left the city soon had cases aboard; persons having the disease and dying of it, were carried to all the landings, towns, and cities up the Mississippi, as high as Cincinnati. In many of these places it spread to a limited extent; in others it did not; in no place did it prevail as an epidemic. It spread among the plantations along the river and in the interior of Louisiana. To some of these the infection appeared to be directly carried; at others it began without any communication with an infected district. It prevailed at Houston, Texas, while Galveston on the sea-board escaped, although on the line of travel from New Orleans to Houston. At the Charity Hospital, as many as 50 cases occurred among the nurses, servants, and patients.

Some other facts are well worthy of attention. The Guttenburg left Hamburg in October, and six or seven deaths occurred before she got out of the Elbe. When she got to sea the disease disappeared, and did not again return. So that in this case, getting to sea out of the infected district arrested the disease.

The analogy between the cases of the New York and the Swanton is certainly remarkable: both vessels left a port in which no cholera case was known, with emigrants; the time of departure was nearly the same, viz. the Swanton on the 2d or 3d of November, the New York on the 9th; both were bound to different ports of the same country, and therefore probably followed nearly the same track; in the Swanton, which sailed on November 2, cholera appeared after twenty-six days, therefore on or about the 28th of November; in the New York it appeared on the 25th of November. In these cases it appears possible that both vessels may have been nearly in the same position, when they were attacked by cholera. Considering, then, all the circumstances of the case, the departure of those ships from a healthy port, the length of time they remained free from sickness, their possible vicinity to each other when they were attacked, the fact of their being emigrant ships, and therefore presumably badly cleaned and ventilated, it does not appear improbable that both ships became nearly at the same time subjected to some peculiar influence, which caused in both cases an analogous and independent manifestation of the cholera poison.

Dr. Fenner, the reporter of the New Orleans epidemic, says, "Whether it be a mere coincidence that epidemic cholera broke out in this city, just at the time when a vessel arrived, having some cases of cholera on board, or that said vessel brought the infection, which rapidly spread through the whole community, is an exceedingly debatable question." And at the end of his Report he remarks, that the few facts he has brought forward do not decide about the contagiousness or transportability of cholera. It would seem, therefore, that the channel of introduction, or the reality of actual importation of cholera, into New Orleans, may yet be a matter of dispute.